STUDIES FROM THE LABORATORY OF EXPERIMENTAL PSYCHOLOGY OF THE UNIVERSITY OF WISCONSIN.

BY JOSEPH JASTROW, PH. D.

I.—THE EFFECT OF FOREKNOWLEDGE UPON REPETITION—TIMES.

(With the assistance of FREDERICK WHITTON.)

The experimental contributions to the study of the effect of foreknowledge upon the times of simple mental processes may be thus briefly summanized. In simple reactions the nature of the stimulus is of course foreknown, but the precise moment of its appearance and its intensity may be left indefinite. It has been found that the omission of a preparatory signal, or an irregular interval between signal and stimulus, as also are irregular variation between more or less intense stimuli, all lengthen the simple reaction-time. In that form of a distinction-time, in which one particular stimulus is to be reacted to but all others are passed without reaction, it is found that the larger the number of possible stimuli (and therefore the less definite the foreknowledge) the longer the reaction-time. In adaptive reactions, with the number of modes of reaction constant the time will be longer as each mode of reaction is connected successively with one, with two, with three or with more and indefinitely many stimuli; the stimuli may or may not be grouped in classes. In association-times Münsterberg has shown that the preceding of a question asking for a personal preference or judgment between a pair of objects, by the mention of a dozen or so of the class of objects to which the pair belongs, decidedly shortens the time of answer to the question, in one series from 947 σ to 676 σ. [1] This last form of experiment is extremely interesting; it seems to show that although we cannot begin to say, for example, whether we prefer peaches to pears, until we have heard the full question,—"apples, plums, cherries, peaches, grapes, oranges, pears, figs, lemons, dates, apricots, pine

[1] For a more detailed account of these points see Jastrow, Time-Relations of Mental Phenomena; pp. 15-17, 39-40, 50-51, etc.

apples,—which do you prefer, peaches or pears?,"—yet the time needed for this decision is much shorter than when the introductory series of words is omitted.

The object of the present study was to test this point in a much more simple type of reaction, and with a variable number of possible stimuli. We selected for this purpose the repeating aloud of spoken words, the operator called a word and as quickly as possible the subject repeated it, all the words used being monosyllables. We found as the average of about 250 experiments with each of us that the time needed for doing this when the word might *be any word whatever*, for J. J. 269 σ for F. W. 267 σ. We formed lists of words as follows: (a) 100 very common verbs, signifying simple actions; (b) 50 common names of animals; (c) 20 proper names, such as John, Frank, Bess, Kate; (d) 20 letters (omitting b, d, m, n, v, w, as confusing in sound or polysyllabic); (e) 10 common French words; (f) the ten numbers, 'one,' 'two,' etc. to 'ten.' Only one list of each class of words was used, so that we became increasingly familiar with the lists. Before each set of 20 experiments the entire list of words from amongst which the words for repetition were to be selected, was read aloud. The following table shows for each of us the average time needed to repeat words under these circumstances. Each result expresses the average of from 240 to 300 experiments.

The conclusion thus corroborated is that *as the range of possible words decreases in extent, as the subject's expectation is more and more definite, the time needed to repeat the word becomes shorter.* It indicates the power and the utility of a general direction of the mind in the line of a more specific operation; the entrance into the general field of attention as preparatory to entering its fovea; the apperception that precedes preception, or in Galton's words, the entrance into the ante-chamber of consciousness to prepare the way for admission to the audience-chamber. We have here an adaptive reaction, each different word forming a distinct stimulus and the vocal manipulation necessary to repeat it a distinct form of reaction. It would seem that we could not get ready to repeat a word until we knew what the word is, and yet a knowledge of the possibilities of the case really aids our expectation and shortens even so simple a process as repetition. We perform the coarse adjustment before the stimulus appears, leaving only the time of the fine adjustment to be measured. There exists all degrees of definiteness and indefiniteness of expectation, of fore-knowledge, and an increase of definiteness to a certain limit brings about a shortening of the mental processes of recognition and adaptation.

While the results are too few and too variable to admit o any detailed treatment a few more special points may be pointed out as suggested though not as established. The extreme regularity of the results, the gradual decrease from repetitions of one of an indefinite number of words, to one of 100, of 50, of 20 and of 10, is doubtless accidental; the times for repeating one of 50, one of 100 or one of an indefinite number of words, for F.W. and of the latter two for J. J. are practically the same, and indicate a limit to the range of expectation. To expect one of 100 words seems scarcely a more definite attitude than to expect any word whatever. With F. W. this seems true of 50 words as well. It seems clear that it takes less time to repeat one of 20 words than one of 50 words, and least to repeat one of the ten numbers. We know the numbers so well as a class and as a series that expectation is here most definite. A French word on the other hand is relatively unfamiliar, and it takes longer to understand and repeat it. To obtain the time needed for the mental portions of the process alone, we substract the simple reaction-times from the repetition-times. How the former was obtained will be explained in a note.

Dr. Münsterberg has attempted to carry the distinction between the "motor" and the "sensory" form of reaction into complex types of reaction; indicating by the former a more special attention to the modes of reaction, by the latter a more special attention to the stimulus. Dr. Martius attempted to repeat the experiments in every detail but failed to obtain the distinction. We found it difficult to maintain this difference of attitude in repeating words, and the results (see the table above) show practically no difference in our cases between the two forms of reaction; the average of all the "motor" experiments was for J. J. 245 σ, for F. W. 249 σ, of the 'sensory' for J. J. 249 σ, for F. W. 251 σ. Even in the simple reaction the difference is slight; but in the ordinary reaction with a finger-key one of us shows the difference. J. J.'s simple reaction to a sound by closing a key with the finger is 136 σ for 'motor,' 162 σ for 'sensory'; F. W.'s 133 σ and 137 σ. While these results have probably only an individual significance, yet in our present incomplete knowledge of the true nature of the distinction between 'motor' and 'sensory' reactions, they may be worthy of record. *Note upon apparatus and method.* Our apparatus and method were gradually perfected during the course of the experiments (covering a period of eight months) and only such of our results were included in the averages given above as were obtained by the same method and seemed fairly comparable with one another. We began by attempting to speak the word and press the key

with the finger at the same moment, the subject also repeating the word and pressing the key as nearly as possible at the same moment. (We used keys of the form to be described in the next note, but later to avoid the noise the caller used a mercury key). This is also Münsterberg's method. We soon found a very strong tendency to close the key too soon, on the part of the reacter, and too late on the part of the caller ; the former presses the key when the voluntary impulse is ready, when he feels that you know what the word is and what it is necessary to do to repeat it, rather than when the vocal mechanism is ready and may act. By this method our times were much too short, centering about 200 σ. The simple reaction time to a sound by closing a key with the finger was for J. J. 148 σ, for F. W. 135 σ. But it is hardly proper to subtract this from the repetion time to obtain the time of the mental process alone. To include the complete mechanical process the stimulus must be a vocal utterance with an accompanying closure of the key with the finger and the same for the reaction. After much trial we conclude to use a small bit of wood held between the teeth and attached to a spring lever, so that the slightest separation of the teeth, (always accompanying utterance) would cause the bit to fly out of the mouth and in so doing to make or break the chronoscope circuit. While the key is not free from objections, it worked very well and we could observe with it no serious tendency to anticipate the true reaction. The simple reaction-times recorded in the table were obtained with this key in the following manner : the observer always uttered the sound "ah" (explosive) and the reacter always used the same sound in reacting, so that the simple reaction includes all the mechanical parts of the process, and whatever error there is in uttering or repeating is contained in all alike. The difference between this and the repetition-time (on the average 68 σ) may thus be regarded as the pure mental repetition process. The further details of method and apparatus offer no peculiarities worthy of record.

A Novel Optical Illusion.

(With the assistance of G. W. Moorehouse.)

If before a rotating disc composed of a large sector of one color and a small sector of another, the two differing considerably in shade (e. g. a dark blue and a light yellow), a rod, held horizontally, be passed up and down, the whole disc seems broken up by horizontal parallel bands of a color similar to that present in greater proportion[1]. This illusion

[1] This illusion was first brought to my notice by Dr. Münsterberg upon my visit to his laboratory at Freiburg. I can find no reference to it in the literature accessible to me.

is especially striking when the component colors are markedly different, with the lighter color forming only a very small portion of the disc, when the disc is in very rapid rotation, the rod very slender and its motion moderately rapid. The bands appear quite as well if the movement of the rod is vertical, oblique, rotary, etc.; the effect of bending the rod into a spiral or other fanciful shape and giving it a rotary movement is especially striking, the bands always following parallel to the outline of the wire. If instead of showing but two colors the disc is composed of three or more the bands appear each composed of several colors ; and if a disc composed of small sectors of the seven primary colors be rotated each band presents a rainbow-like appearance. This phenomenon seems especially remarkable when contrasted with the universal tendency of successive optical images to fuse. The mixing of colors upon a disc is itself a typical instance of such fusion. But here there is a sort of separation of colors, and that too at a high rate of rotation. For example, if two rotating discs were presented to us, the one pure white in color, and the other of ideally perfect spectral colors in proper proportion, so as to give a precisely similar white, we could not distinguish between the two ; but by simply passing a rod in front of them and observing in the one case but not in the other the parallel rows of colored bands, we could at once pronounce the former to be composite, and the latter simple. In the indefinitely brief moment during which the rod interrupts the vision of the disc, the eye obtains an impression sufficient to analyze to some extent into its elements this rapid mixture of stimuli. The more detailed description and possible explanation of this illusion formed the object of our study as of the present exposition. It will conduce to brevity of description to arrange the several results of experimentation under appropriate headings.

Extent of the Illusion. The illusion appears with any pair of colors, provided only that the two are moderately different; but the resulting bands are of various degrees of distinctness according to the colors used. The result is clearest when the colors are strongly contrasted ; we experimented successfully with red, yellow, blue, green, black, white, etc., in various combinations. Of a series of seven shades of green, numbers "one" and "two" were very dark, number "three" considerably lighter than "two," and the rest all very light with only slight differences between them. The bands could not be observed with a combination of "one" and "two" nor with any combination of "four," "five," "six," or "seven," but in all other combinations the contrast was sufficient to cause the illusion. By a differ-

ent method, to be described below, we succeeded in more accurately determining the amount of contrast needed to produce the illusion.

Proportions of the Component Colors. In a disc composed of dark red and light yellow, the bands could just be seen when a sector of 12° of red was combined with 348° of yellow, and remained visible with a decrease of yellow and an increase of red until only 3° of yellow and 357° of red were present. With red predominating the bands are also red but of a red *darker* than the general color of the disc; with yellow predominating the bands are yellow but of a yellow *lighter* than the resulting mixture. The darker bands are always more easily seen and clearer than the light ones, and hence a smaller sector of yellow with red than of red with yellow is needed to produce the illusion. We should infer that there would be a ratio of the two colors at which the bands would be neither darker nor lighter than the background; and in fact there is quite a range of ratios for which the bands are so nearly the color of the background that they are difficult to observe. This range differs for different combinations of colors ; for our red and yellow the critical point is about 110° of red and the rest yellow. With more red than this the bands become more and more deeply red, and with less red more yellowish ; to this extent the statement that the bands are of the color predominating in the disc must be modified.

Effect of the Width and the Rate of Motion of the Interrupting Rod. The general effect of an increase in the width of the interrupting rod was to render the illusion less distinct and the bands wider ; moreover the illusion is more limited in range, i.e., it is confined to a narrower range of rotation rates of the disc and the like. While with a fine wire about a millimeter in diameter, the bands are sharply outlined and striking, with a stick 4 mm. in width they require somewhat of a strain to continuously observe them.

Maintaining the rotation rate of the disc as nearly constant as the clockwork that runs it will allow, we may vary the rate of passing the rod to and fro with characteristically different results. Moving the rod across the disc six inches in diameter, so that each movement from up down, and from down up, corresponded with the beat of the metronome beating 208 times per minute, the bands were about $\frac{5}{8}$ inch apart, with the metronome at 160 per minute about $\frac{1}{2}$ inch apart ; with 108 per minute $\frac{1}{4}$ in.; with 80 per minute $\frac{1}{8}$ inch ; with 60 per minute less than $\frac{1}{16}$ inch. In other words the bands are separated by smaller and smaller spaces as the rate of movement of the rod becomes slower and slower. The distances

between the bands were estimated by free-hand drawings and then verified by comparison with the rotating discs.

Analysis of the Factors of the Illusion. Allowing the above to suffice as a general explanation of the extent of the illusion, we may proceed to an analysis of its component factors. The factors are (a) the appearance not of one band but of several; (b) the distance between the bands; (c) the color of the bands; (d) the width of the bands; (e) the color of the interrupting rod; (f) the width of the interrupting rod; (g) the rate of movement of the interrupting rod; (h) the rotation-rate of the disc; (i) the nature and proportion of the component colors. It will thus be seen that the illusion is quite complicated. As an important step towards its explanation, we will consider first,

The Time-Relations of the Illusion. This involves the factors (a), (b), (g), (h). Before proceeding further it will be necessary to describe another method of producing the illusion, without which its explanation would have been impossible. This consists in sliding two half discs of the same color over one another leaving an open sector of any desired size up to 180° and rotating this against a background of a markedly different color, in other words we substitute for the disc composed of a large amount of one color, which for brevity we may call the "majority color," and a small amount of another, the "minority color," one in which the second color is in the background and is viewed through an opening in the first. With such an arrangement we find that we get the series of bands both when the wire is passed in front of the disc and when passed in back between disc and background; and further experimentation shows that the time relations of the two are the same[1]. (There is of course no essential difference between the two methods when the wire is passed in front of the disc.) These facts enable us to formulate our first generalization, viz., that for all purposes here relevant the seeing of a wire now against one background and then immediately against another is the same as its now appearing and then disappearing; a rapid succession of changed appearances is equivalent to a rapid alternation of appearance and disappearance. Why this is so we are unable to say, but the fact itself seems well established, and is both

[1] Of course when the wire is passed between the disc and background the distinctness of the wire depends upon its contrast with the background; it appears of its true color modified by its appearance on the background and by the rotating disc through which it is seen, but it does not assume the contrast effects assumed by the rod moved in front of the disc. The time-relations in the two cases are the same but the color-phenomena considerably different.

novel and interesting. By this "open disc" method we are enabled to study the illusion independently of color, by having the disc of white against a white background with the rod moving between disc and background. In this case, as in the others, we see several rods or bands, and the suggestion is natural that we are dealing with the phenomena of after images ; in other words we see the rod through the opening in a certain position, then for a brief time lose sight of it, then see it again in a slightly different position and so on, the after image of the one view not having faded out when the second view is obtained. If this is the true explanation of the fact that several rods are seen, then we should—with different rotation rates of disc and rod—see as many rods as multiplied by the time of one rotation of the disc would yield a constant, i.e., the time of an after image of the kind under consideration. The result of about 20 such tests with varying rates of the disc was the following :

	J.J.	*G.W.M.*
Average time of rotation of disc when 2 images of the rod were seen	.0812 sec.	.0750 sec.
" " " " " 3 " " " "	.0571 "	.0505 "
" " " " " 4 " " " "	.0450 "	.0357 "
" " " " " 5 " " " "	.0350 "	.0293 "
" " " " " 6 " " " "	.0302 "	.0262(1)"

Multiplying the number of rods by the rotation rate we get for J.J. an average time of after image of .1740 sec. (a little over $\frac{1}{6}$ sec.) with an average deviation of .0057 (=3.2%); for G.W.M. 1492 (a little over $\frac{1}{7}$ sec.) with an average deviation of .0036 (=2.6 %). An indepentent test of the time of after-image of J.J. and G.W.M. by observing when a black dot on a rotating white disc just failed to form a ring resulted in showing in every instance a longer time for the former than for the latter.[2]

It has already been observed that the distance between the bands diminishes as the rotation rate and the rate of movement of the rod increases ; this suggests that the distance between the parallel bands is that moved over by the rod during one rotation of the disc. This we tested with various rates of disc and rod by spreading a pair a compasses until they seemed to span the distance between the bands. The following is a comparison of the actual and the theoretical result under this hypothesis :

---

[1] There is a further point to be considered here, viz.: the size of the aperture, when nothing different is said it was 21°. We repeated some of the above experiments, however, with apertures of 10½° and of 42°, obtaining the same results.

[2] For the method of timing the disc see Note A. The rod was moved between parallel bars to the beats of a metronome.

One revolution in x seconds.	Rod moved y mm. in x sec.	Observed distance between bands in mm.
.0551	18	18
.0220	5.13	5
.0227	5.03	5
.00987	1.48	2
.0233	3.04	3.5
.0250	3.05	4
.0376	5.03	6.5

Considering the difficulties of the observation these agreements are extremely close. Having now accounted for the width of the bands, the distance between them, the fact that several are seen, it remains to examine certain general conditions of the illusion and more particularly the color factors of it.

The Color Factors of the Illusion. A brief acquaintance with the illusion sufficed to convince us that its appearance was due to contrast of some form, though the precise nature of this contrast is the most difficult point of all. It has already been observed that the two component colors must be somewhat different to produce the illusion and that the bands are darker when the majority color is darker than the minority color, and is lighter when the former is lighter. By the following device we succeeded in determining the minimum amount of difference between the colors that would produce the illusion: we used an open disc of light green (aperture 21°) in front of a back ground of the same color and used with the green disc a variable sector of black. When moving a rod in front of this combination we always observe a series of light bands due to the presence of the large amount of green with a little black, but as the black gains a certain proportion, we observe in addition a series of dark bands due to the contrast of the resulting darker green (mixture of light green and black) with the light green of the back ground. We have now only to vary the black till these darker bands may just be seen; this critical point with the colors used proved to be about 24° of black added to 315° of green, or "$\frac{1}{13}$ darker" if we may use that expression. It need hardly be added that the aperture exactly corresponds to the minority color and requires no special consideration.

Colors differ in two senses, in the fact that they are formed by different wave lengths, and that they contain more or less black. We have shown that colors differing only in the latter respect produce the illusion; it remains to be seen whether difference of color alone will produce it. We have the following evidence that it will not: (1) We were able to select a dark red and a dark blue, which did not give the illusion, but in which the substitution of slightly different red or blue, brought it out; (2) the same is true of a light green and light yellow; (3) in many cases while not succeeding in obtaining

colors that would cause the illusion to disappear, we succeeded in finding for any given color a second with which the illusion is faint, and (4) we can effect this more systematically by combining with one of a pair of colors yielding the illusion sufficient white or black to cause it to vanish. In a vague and popular sense we call a given red lighter or darker than a given blue, but the physicist (as we understand it) has no accurate determination of this impressionist estimate; perhaps for ordinary empirical purposes it would be of advantage to call two colors equally dark when they fail to give the illusion now under discussion.

There is a factor in the illusion not yet considered, viz., the color of the interrupting rod. Heretofore this has been a copper wire; and whenever the illusion is distinct the color of the wire is of very slight importance, but when it becomes difficult to observe, then wires of certain colors will produce it and of others will not. The general outcome of much experimentation with colors hardly sufficiently contrasted in shade to produce the illusion is this, that with the component colors both rather dark, whether in proportions giving a light band or a dark one, dark wires will produce it, but light ones will not, with the component colors both light, light wires will produce it but dark ones not.[1] We are unable to bring this result into harmony with the ordinary laws of contrast, and must be content to give the empirical result without explanation.

We have but one further mode of observation that sheds light on the present point. We can obtain the illusion quite as well by substituting for the disc a cylinder covered by a strip of colored paper with a small strip of another color crossing it. We happened to use a rubber band to hold the second strip in place and noticed a deep contrast band parallel to the rubber when in rotation. We substituted a lead-pencil mark for the rubber and still obtained the deep band, *this band being of the same color as the bands* produced by passing a rod before the disc or cylinder. A lead-pencil mark on the disc will have the same effect. We observed however that this appeared only when the line passed across the color present in lesser proportion, which at once suggested (conformably to the experiment with the open disc) that the bands are originated during our vision of the minority color. We tested this by fixing a strip of brass to the disc in such a way that it could be made to rotate on its own centre (by striking against a fixed point) during the rotation of the disc. This device replaced the rod and caused the illusion so long as it

[1] The different colors of wire were simply insulated wires with the colors of the insulation different.

was fixed to the minority color but not when fixed to the majority color. This offers some clue to the kind of contrast involved in the illusion but still leaves room for a satisfactory explanation.

The chief points of our study may be thus resumed:

(1.) The illusion appears whenever the component colors are moderately contrasted in shade, and the one is present in distinctly greater proportion than the other; a difference in color, but not in shade, does not produce the illusion.

(2.) For all purposes affecting the illusion (except certain points of color) alternate appearances of an object against different back grounds is equivalent to alternate appearance and disappearance of the object.

(3.) The fact that several bands are seen is due to the persistence of the after image.

(4.) The distance between the bands is the distance through which the rod has passed in one revolution of the disc.

(5.) With the majority color darker than the minority color the bands are darker than the resulting mixture, and lighter when the majorily color is the lighter.

(6.) The width and rate of movement of the rod as well as the rotation-rate of the disc determine the width of the band; the color of the rod becomes important when the illusion is difficult to obtain, it then appearing that with the dark colors a dark rod is better, with light colors a light rod.

(7.) The bands originate during the vision of the minority color.

(8.) The contrast effect of the bands (while not satisfactorily explained) may also be obtained by a mark upon the minority color.

ACCESSORY APPARATUS FOR ACCURATE TIME-MEASUREMENTS.

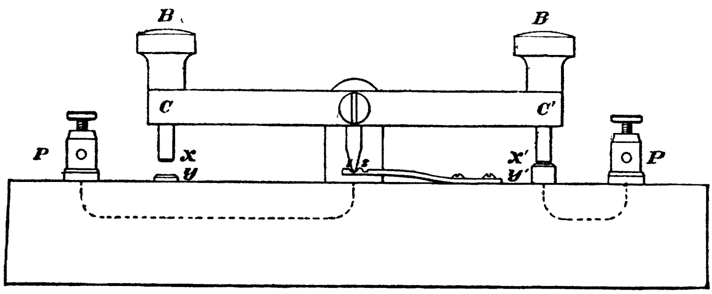
(With the Assistance of FREDERICK WHITTON.)

A large portion of the measurement of the times of mental processes has been accomplished with the aid of the Hipp chronoscope; the objections to the use of this apparatus are the difficulty of its regulation and the possible sacrifice of accuracy to convenience. To secure accuracy, the chronoscope must be constantly controlled, and for this purpose complex devices have been resorted to for ensuring constancy of conditions and the like.[1] To simplify the method of control we

[1] The apparatus supplied with the chronoscope is altogether defective, the mechanical release of the ball is bad, the measurement of the falling height uncertain, the catch by which the circuit is closed imperfect and slow. In addition to these mechanical defects, the range of height is much too meagre. See Wundt's note to the same effect. Phys. Psych., 3d ed., II. p. 276. Note.

succeeded after many trials and failures in perfecting a piece of apparatus by which the error of the chronoscope could be readily and accurately determined. In all methods in which the control consists in timing the fall of a ball (and this too is our method), the general problem is this: two pairs of events are to occur simultaneously, first the ball to begin its fall and the hands of the chronoscope to move, and then at a definitely measurable point of the fall, the hands of the chronoscope are to stop. The difference between the present and all other methods of securing this end (and to which we think its success is due) is that this simultaneity is obtained not by allowing a magnet to release a ball and also a blade held against the action of the spring or the like, but by the use of a special form of key. A simple movement of this key serves to *make* one of two independent circuits and to break the other at the same moment. The explanation of this key will be easier if preceded by a description of the form of key used almost exclusively in this laboratory for finger reactions. It is a key that when once closed remains closed and when opened remains open; this gives the advantage of having the closing and opening movements the same, and allows this movement to be the very natural one of quickly tapping the key and then withdrawing the hand. A brass arm CC' pivoted at its centre upon a brass upright terminates above at each end in a hard rubber button BB', and below in a brass point XX'; projecting from the board upon which the whole is mounted are two brass points YY' for the purpose of making or breaking contact with XX'; finally there is fastened to the arm CC' a wedge-shaped piece of brass playing between the notches 1, 2. The key as pictured is ready to be used to break a circuit, made through the point X' connected with the binding-post P', and the point central support connected through the apparatus with the binding-post P. A simple pressure of the finger at B' breaks the contact at X' Y', and forces the wedge into the position (2), in which it is securely held by the notch (2). When in this position it is ready to be used to *make* a circuit by a pressure upon B'; it can only assume one or the other of these two positions, and in either case is securely held in place. Now imagine that the button B' instead of being of hard rubber is of brass, and imagine the end of a second brass lever at right angles to CC' in position to press down upon B' and thus establish a circuit between B' and the second lever; imagine further that the blow of this second lever upon B' is given by the release of a strong spring that holds everything firmly until X comes in contact with Y, and the apparatus is complete. A release of the spring thus establishes one circuit through B' and the second lever which sets

the chronoscope going, and at the same moment the ball begins to fall by the breaking of the circuit at $X' Y'$. The circuits are entirely independent and supplied by different batteries. To test the simultaneity we make our connections so that the making of the one circuit sets the chronoscope going and the breaking of the other stops it; and in no case did the chronoscope hands show the slightest tendency to move.



The apparatus controlling the fall of the ball is simple. An electro-magnet tapering to a point at one end is tightly held in a bracket, adjustable along a vertical slide, which in turn is securely fastened to the window frame. It is important that all parts of this be strong and securely fixed to the wall of the building. The slide is 6 to 7 feet high so that a fall of .6 to .7 seconds can be measured. From the value of g at Madison we calculate from the formula $s=\frac{1}{2} g t^2$ the heights at which the ball should just consume .1, .2, .3, .4, .5 and .6 seconds in its fall and mark these points on the millimeter scale along the slide, making our readings by aid of a fine wire. The ball of soft iron about $\frac{3}{4}$ of an inch in diameter is held at the tip of the magnet and in its fall strikes against the arm of a well-balanced lever, and thus severs an electrical connection by which the clock comes to a standstill; while the distance between the upper surface of the lever and the lower surface of the ball is the space fallen through in the measured time. Two further points may be noted; first to find the zero point on the scale let the magnet hold the ball and move the bracket down until the ball just touches the lever sufficiently to break the connection, and mark the point opposite the wire zero; second, use three or four thicknesses of tissue-paper between the ball and the magnet to separate the surfaces and thus diminish the time of demagnetization. With this apparatus one can without assistance take half a dozen records at different points in as many minutes; and in the work described above ten observations were recorded before and after each day's work, from which the

error of the chronoscope for the day was calculated. As the observations were taken from all six positions—.1 to .6 sec. (in the latter portion of the work for four positions) we could determine whether the error was constant or relative and found the former to be the case. Throughout a period of six months, during which the chronoscope was tested, its maximum error was .005 seconds and the average error about .002 seconds. The position of the springs regulating the chronoscope was always noted and by changing these the error could be reduced to practically zero. But we aimed not at absolute accuracy but at an accurate determination of our daily error. This apparatus has proved itself so easy of manipulation and so time-saving, that its use is confidently recommended to experimental psychologists.

Note A.—On the Timing of Rotating Discs. A simple and fairly accurate means of determining the rate of rotating discs, especially of those rotating by clockwork, has long been a desideratum. The ordinary speed-counter is out of the question on account of the great friction involved. The "interruption-counter" invented by Dr. Ewald of Strassburg is a device by which each making of an electrical circuit moves the hand of a dial just one division, the dial showing 100 divisions; its original purpose was to count the vibrations of a tuning fork and thus to serve as a convenient form of chronoscope. It is capable of counting the vibrations of a fork with a vibration-rate of 100 per second, but for this, great delicacy of manipulation is necessary. Its adaptation to the present purpose is simple, though quite a number of devices were attempted before the simple one was obtained. Two small platinized tips were soldered at opposite points on to the circumference of the wheel of the clockwork next to the one to the axle of which the disc is attached. A light brass blade, also platinized, is suspended from above with a thumb-screw regulation, so that the tips on the wheel just make a coutact with it as they pass it. As this second wheel revolves once to every eight rotations of the disc we can count to the nearest four rotations, which is quite accurate enough. By increasing the tips we can count every two or every rotation, though the adjustment must then be finer. We allow the current to run through the counter for 15 seconds (as counted by the second hand of a watch) by closing and releasing a mercury key. We also devised a method by which the timing was done automatically and so one person could observe the disc and take the time measurements as well. This consisted in fastening to the ends of an ordinary revolving drum a circular piece of paper with a strip extending over about 180° cut out; by placing the end of a fine wire

opposite this paper it is easy to arrange one's circuits so that during the time the wire touches the brass of the drum the counter is recorded, while for the rest of the revolution of the drum the current is intercepted by the paper; finally we set the drum so that the time of contact is a convenient one, say 15 sec., and when we see the contact approaching close, we lock the key and go on with our observation. The counter then of itself begins to record, does so for exactly fifteen seconds and stops; and we can make the reading at our leisure. For all these purposes the counter proved itself an exceedingly valuable apparatus.

As this is one of the first of these instruments to be used, our experience with it may be of advantage to others. Its two defects are that the wire on the magnets is too fine, thus causing an excessive resistance, and that the spring by which the magnet blade is withdrawn is not adjustable. After remedying these defects we were able to successfully manipulate the instrument with a single storage cell battery and very little trouble. We also tested the apparatus with a tuning fork of 100 per second and found it reliable. If the instrument were made as large again its efficiency would be increased.

Note on a device for color mixing. One objection to the ordinary method of mixing colors by forming sectors of them upon a disc and rapidly rotating it, is that while the mixture is produced one cannot readily compare the result with the original component colors. It is as a corrective of this defect that the following device is suggested. It consists simply of using a half disc (or any other desired portion most easily obtained by sliding two half discs or four quarter discs upon one another) of the one color and during its revolution holding the other color in back of it to one side. Then on either side you have the original colors, and where the two overlap the resulting color; if the colors be red and blue, you have before you on either side the red and blue and between them a purple. One can hold two (and with proper arrangements more) different colors in back of the same rotating disc and thus show for instance the mixture of blue with red and blue with yellow and the original blue, red and yellow all clearly displayed in line. One can show the mixture of an entire series of colors with the same color without stopping the disc, and for matching a given color with a resulting mixture this is especially convenient. With two rotating discs, overlapping upon a common background one can show the result of mixing three colors and the three original colors at the same time, but there the manipulation is no longer so simple. The method is easily adapted to the

fusion of other visual impressions and is particularly suited to class-room demonstration. A clockwork for rotating the disc is a great convenience in the experiment.

THE PSYCHOPHYSIC SERIES AND THE TIME-SENSE.

(With the assistance of W. B. CAIRNES.)

In an earlier paper reporting the studies made in this laboratory (AMER. JOURNAL OF PSYCH., Vol III, No. 1, pp. 44-49) it was shown that when we attempt to sort out sizes of sticks into six or nine magnitudes, either by the eye or by passing the finger over the sticks, the result is that the average lengths of the sticks of the several magnitudes are separated by approximately equal differences; i. e., they form an arithmetical series. This method was spoken of as that "of the psychophysic series," and consists simply in distributing according to a general impression a large variety of sense-impressions into classes or magnitudes; it is also the method by which the stars were divided into their magnitudes. If the psychophysic law holds when thus tested the result would be, as it notably is, in case of the stars[1] that the ratios of the averages of neighboring magnitudes would be constant, i. e., would form a geometrical series. A suggestion of an explanation of the applicability of the law to star magnitudes and its failure in magnitudes of extension both visual and tactual-motor, was recorded in the former study in the following words: The law may be expected to apply to "such sensations as are appreciated *en masse*, and with not too distinct a consciousness of their intensity [or extension]; when the sensation is a sort of impressionist reception of the gross sensation without dividing it up into units, or conceiving it as so composed, we may expect the law to hold good. This would be the case with the rough estimations of star brightnesses." To further test this point of view we experimented with the perception of time-intervals, in which as in the estimation of star magnitudes there is an unanalyzed appreciation of the interval, without regarding it as composed of constituent units; and for which, according to the above suggestion, the law should hold good.

Accordingly we set a metronome at one of many intervals and asked the observer after he had listened to its beating as long as he desired in order to determine his judgment, in which of *six* classes of intervals he would place it. At the outset the observer was allowed to listen to the slowest interval, 40 per minute, and to the fastest, 208 per minute, and to

---

[1] See the proof of this in an article Vol. I, No. I, p. 112 of this JOURNAL.

imagine this range divided up into six grades or magnitudes. At first the assignments are somewhat vague and variable but they soon became relatively fixed, though there is considerable overlapping of the various magnitudes even in the best observers. Much of this is undoubledly due to contrast, an interval following a very long one seeming shorter than it would if following a short one, and the like. We used intervals rising by 2 per minute from 40 to 120 per minute, by 3 per minute from 120 to 144, and by 4 per minute from 144 to 208, thus using in all 63 intervals. These were written on small square cards and three sets of such, or 189 cards, were used at one sitting with each observer, the cards being tossed in a box and drawn at random, and the metronome set according to the number drawn. The longest time intervals, i.e., from 1.5 seconds down were called magnitude I, and the shortest from .29 seconds up, magnitude VI; the observer sat with his back to the metronome, knew nothing of the experiment except what were the longest and the shortest intervals, and simply called out the number of the class to which he assigned the given interval. Three such full sets of nearly 200 observations were made on one observer, two each upon two others, and one each on three others, making ten in all. When the results are obtained we collect all the intervals assigned to each of the magnitudes and find the average duration of the magnitudes of that interval, which averages will serve as the basis for the present discussion.

In the accompanying table are shown for each set of observations the average number of beats per minute of each magnitude, with the number of observations contributing to that average following it in small figures the successive differences and ratios of these magnitudes, and the average of these differences and ratios together with the average deviation from them expressed in percentages. At the foot of the table a similar series of weighted averages (i. e., results of multiplying each average by the number of observations and dividing by the total number of observations), is given, combining all the observations, and this we shall first consider. To test whether the series approaches an arithmetical or a geometrical series, we have simply to compare the *constancy of the differences with that of the ratios.* This may be done with sufficient accuracy for the present purpose by finding for each of the five results the differences from their average, dividing this by five, and expressing it as a percentage of the average of the five differences, or the five ratios. We thus see that while *the average variation from a constant difference is 24.8 per cent., the average variation from a constant ratio is only 4.2 per cent., indicating a decided approximation to a*

Group	Magnitudes.	I.	II.	III.	IV.	V.	VI.	Average.	Average Deviation	Ratio.
I.	W.B.C.	50.1^{29}	73.0^{29}	94.6^{30}	112.5^{34}	142.2^{28}	181.8^{37}			
	Differences,	22.9	21.6	17.9	29.7	49.6		28.3	31.9%	1 : 6.51
	Ratios,	1.437	1.299	1.190	1.264	1.330		1.304	4.9%	
II.	W.B.C.	44.5^{23}	67.5^{29}	95.1^{44}	119.0^{35}	151.0^{24}	185.2^{32}			
	Differences,	23.0	27.6	23.9	32.0	34.1		28.1	13.0%	1 : 1.71
	Ratios,	1.501	1.423	1.251	1.274	1.226		1.335	7.6%	
III.	W.B.C.	48.1^{21}	67.9^{29}	96.0^{47}	115.9^{42}	160.8^{28}	190.8^{22}			
	Differences,	19.8	28.1	19.9	44.9	30.0		28.5	24.0%	1 : 3.20
	Ratios,	1.412	1.414	1.207	1.388	1.187		1.322	7.5%	
IV.	J. J.	48.0^{21}	64.1^{26}	89.2^{33}	106.4^{35}	140.5^{39}	186.1^{33}			
	Differences,	16.1	25.2	17.2	34.1	45.6		27.6	35.4%	1 : 9.57
	Ratios,	1.335	1.392	1.193	1.320	1.325		1.313	3.7%	
V.	J. J.	50.6^{35}	77.7^{31}	97.6^{30}	118.5^{30}	146.0^{26}	184.1^{36}			
	Differences,	27.1	19.9	20.9	27.5	38.1		26.7	18.8%	1 : 2.57
	Ratios,	1.535	1.256	1.214	1.232	1.261		1.300	7.3%	
VI.	R.H.T.	44.2^{9}	57.9^{22}	74.0^{27}	92.2^{44}	131.4^{42}	176.3^{44}			
	Differences,	13.7	16.1	18.2	39.2	44.9		26.4	48.1%	1 : 12.66
	Ratios,	1.310	1.278	1.246	1.425	1.342		1.320	3.8%	
VII.	R.H.T.	44.4^{10}	54.0^{17}	81.8^{44}	105.0^{44}	132.7^{33}	178.9^{39}			
	Differences,	9.6	27.8	23.2	27.7	46.2		26.9	31.2%	1 : 4.73
	Ratios,	1.515	1.216	1.284	1.264	1.349		1.326	6.6%	
VIII.	F. St. W.	48.1^{20}	69.4^{39}	98.0^{34}	123.6^{37}	157.6^{36}	183.1^{23}			
	Differences,	21.3	29.6	25.6	33.4	26.1		27.2	12.6%	1 : 1.70
	Ratios,	1.443	1.427	1.261	1.270	1.166		1.313	7.4%	
IX.	S.S.B.	49.1^{24}	69.0^{31}	95.3^{46}	122.6^{35}	158.7^{28}	193.0^{23}			
	Differences,	19.9	26.3	27.3	36.1	34.3		28.8	17.8%	1 : 3.87
	Ratios,	1.405	1.381	1.286	1.294	1.216		1.316	4.6%	
X.	S.D.J.	49.8^{29}	77.1^{33}	98.2^{35}	120.3^{40}	153.7^{22}	188.9^{29}			
	Differences,	27.3	21.1	22.1	33.4	35.2		27.8	18.6%	1 : 2.58
	Ratios,	1.548	1.274	1.225	1.277	1.223		1.308	7.2%	
Weighted Average.	Weighted Average.	$50.0^{22\ 1}$	$69.6^{28\ 6}$	$93.6^{37\ 0}$	$114.3^{37\ 6}$	$147.7^{30\ 6}$	$185.1^{31\ 8}$			
	Differences,	19 6	24.0	20.7	33 4	37.4		27.0	24.8%	1 : 5.90
	Ratios,	1.392	1.345	1.221	1.292	1.253		1.301	4.2%	

geometrical series, and therefore, according to expectation, an obedience to the psychophysic law. In the last column of all, the ratios of each pair of average deviations are given, and for the general result (accepting this rough mode of comparison), we have this, namely, that the approximation is six times (5.90) as close to a geometric as to an arithmetic series.

We may instructively note too a few peculiarities of these results; first, that while the ratios of neighboring magnitudes are approximately constant, there is a tendency for these ratios to decrease slightly from I to VI, or to increase in passing from short intervals to long ones. A precisely similar result is found in the case of star-magnitudes; and in the latter case the observations are sufficiently extended and regular to warrant an emperical formula expressing the rate of increase of this ratio, with an increase in brightness of the star-magnitudes. Moreover, two further irregularities recorded in the study of star-magnitudes reappear in the present study. The first is that at one extreme the ratio tends to be unusually large, and at the other unusually small. This is due to the limitations of the series, and the fact that were there another magnitude at each end of the series, some intervals now placed in I or VI would be placed in the class below I, or in that above VI. The errors thus induced are evidently opposite in direction. The tendency is more marked in the star observations than here, but if we note the individual results we see that in seven of ten cases the ratio of I to II is markedly larger than the others, and in five cases the ratio of V to VI is appreciably smaller than the others. These peculiarities are good evidence of the similarity of the psychological processes employed in sorting stars and in classifying time-intervals with magnitudes. A marked peculiarity of the present series (and one that interferes seriously with its regularity), is the tendency to make only a slight division between intervals assigned to III and those assigned to IV, but a marked one between those assigned to IV and those to V. This tendency is present in nine of the ten sets, and is marked in six, and so can hardly be accidental. It seems to depend upon a habit of viewing III and IV as medium intervals, while V is already a short interval. A closely similar irregularity was found in the estimations of the star-magnitudes of Ptolemy and Sufi.

Regarding the individual results we notice considerable irregularity, some individuals maintaining the law much more closely than others, as is observed most readily by a view of the last column of the table. That much of this irregularity is due to the paucity of observations is indicated by the fact that the average deviations in the combined sets I, II, III, of

W. B. C., IV, V, of J. J. and V, VI, of R. H. T., are smaller than the average of the group of three or of two sets. Thus for W. B. C.'s three sets the average variation from a constant ratio is only 4.8 per cent., in J. J.'s two sets 4.2 per cent., in R. H. T.'s two sets 3.3 per cent., while the ratios of the average variations from a constant difference and a constant ratio becomes as 1:5.54, 1:6.62 and 1:10.97. It should also be noted that the number of intervals assigned to each magnitude differs considerably. In the general average the deviation from the average of 31.3 for each magnitude is 13.4 per cent. III and IV have most intervals assigned to them (perhaps because many doubtful ones are naturally assigned to the medium magnitudes). I and II have fewest.

One further point may be mentioned as supporting the supposition that with a more conscious analysis of time-intervals, with the establishment of a habit of estimating time by seconds, the tendency to follow the geometrical series will be diminished. Thus it is quite noticeable that the first sets of all three observers who went through more than one set approach more closely to the psychophysic series than the later ones, the average deviations in the two cases being about as 4 to 7. Perhaps this is accidental, but it certainly suggests a departure from the impressionist method of estimating intervals with which we set out. Of the remaining three records VIII is unsatisfactory and was so noted at the time, while IX and X are records of observers accustomed to astronomical work, in which the second and half-second interval is important. The acquired habit of analyzing time intervals according to standard units may thus account for their slight tendency to follow the psychophysic series in their case.

The result of the present study thus goes to support the suggestion that when we estimate sensations roughly and on general impressions, without comparing them with standard units, we naturally, though unconsciously, make use of a geometrical series. We make relative distinctions rather than absolute ones, and this is the natural basis of the psychophysic law. While the process is a rough one, and is often accompanied by much hesitation and little confidence, the average results are fairly clear, and add one more to the many illustrations of the statistical regularity of apparently lawless and entirely unconscious mental tendencies.

THE PSYCHOPHYSIC SERIES AND THE MOTOR SENSE.

(With the assistance of AUGUSTA A. LEE (MRS. CHARLES GIDDINGS).

As a further application of the method of the psychophysic series we experimented with a form of movement in which with the forearm supported at the elbow as a pivot the hand

moved laterally for practically any distance from 5 to 190 millimeters. The extent of the movement was limited and measured in the following way: The hand held a glass pencil and supported the same along a straight edge, the pencil furthermore moving in the ridges of very finely grooved glass. Over this glass was mounted a skeleton triangle about 6 inches across the base and 20 inches in altitude, and the whole moved in a slide to or away from the hand holding the pencil, such movement limiting the pencil to movements of varying length between the sides of the triangle and parallel to its base. A scale at the side indicated for each position of the triangle the distance moved over by the pencil. After allowing the subject to move over the shortest and the longest distances he was asked to mentally divide this range into six classes or magnitudes, and assign the various distances presented according to the perceptions gained through the sense of motion (sight was of course excluded), to the various magnitudes. Though the average lengths of these magnitudes present considerable irregularity, they very clearly show that they do not accord with the psycho-physic law and that they roughly approximate an arithmetical series. The averages themselves, together with the number of observations contributing to the average, are given in the following table:

	I.	II.	III.	IV.	V.	VI.
A. A. L. (1)	14.8 (47)	40.5 (45)	75.6 (28)	100.6 (18)	135.8 (21)	166.8 (20)
A. A. L. (2)	20.9 (42)	58.5 (34)	93.7 (26)	123.4 (15)	152.0 (12)	169.5 (13)
E.	13.5 (16)	36.6 (20)	70.7 (25)	110.7 (16)	134.8 (17)	168.6 (5)
H.	15.7 (27)	48.3 (28)	80.3 (28)	121.4 (18)	156.5 (13)	181.0 (8)
J. (1)	9.6 (22)	30.5 (44)	60.6 (45)	89.1 (33)	120.4 (24)	144.8 (39)
J. (2)	7.8 (13)	25.0 (25)	53.6 (30)	84.6 (25)	112.5 (13)	150.9 (34)

To show how far these results favor an arithmetical and how far a geometrical series it will perhaps be sufficient to state the average deviation from a constant difference and from a constant ratio of each of these series, expressed as a percentage of the average difference and the average ratio of neighboring pairs of results.

	L. (1)	L. (2)	E.	H.	J. (1)	J. (2)
Average deviation from a constant ratio—	29.8	30.7	31.1	32.5	34.9	33.7
" " " " " difference—	1.33	17.9	19.3	19.0	12.3	16.6

This shows about twice as close an approximation to an arithmetical as to a geometrical series. If however, we take the average of all six series of each magnitude we obtain a much more pronounced obedience to an arithmetical series; the successive differences become 26.2, 32.5, 32.6, 30.2, 28.3, and the average deviation of these from a constant ratio is but 8.6 per cent. of their average value. Finding the average deviation from a constant in all six series we find no such reduction. It is 30.9 per cent.

If we take into account the varying number of observations contributing to each average by weighting each difference with half the sum of the number of observations of the two averages, the difference of which is expressed, we obtain a still closer approximation to an arithmetical series. For the various series the average deviations from a constant difference then become in percentages:

L (1)	L (2)	E.	H.	J. (1)	J. (2)
10.8	18.1	19.0	13.1	12.3	17.4

and for the combined average of all only 6.3 per cent. It may be noted too that the combination of L's as of J's two sets of observations conform more closely to the arithmetical series than either one, and that the greatest deviations from the constant ratio are apt to occur in the two extremes of the series, when the shortest and when the longest lengths are con cerned, the reason of which is obvious. Incomplete as these results are, they are perhaps sufficiently definite to suggest strongly the inapplicability of the physo-physic law (when thus tested), to spatial impressions gained by fore-arm movements. These movements are not altogether dissimilar to those made in running a finger along a stick (v. these studies in this JOURNAL, Vol. III, p. 47), and in both cases the judgment of length is rather conscious and referred more or less definitely to units, probably to notions gained through knowledge of feet and inches. They thus form an additional corroboration of the generalization that we conform to the requirements of the psycho-physic law in gross, *en-masse*, analyzed impressionist judgments, but not in precise detailed, analyzed and considerate judgments. The experiences of a civilized environment have transferred many forms of sense-judgment from the former to the latter class, among them spatial judgments both visual and motor. In these, absolute differences become of equal, and, at times, greater importance to us than relative ones.

THE INTERFERENCE OF MENTAL PROCESSES — A PRELIMINARY SURVEY.

(With the Assistance of W. B. CAIRNES.)

The general field with which the present study deals, (though in a somewhat eclectic and tentative manner), is the power of carrying on two mental processes at the same time. How far, we naturally ask, is this possible, how far economical? How shall we conceive this mental simultaneity, how cultivate and develop the power? We know that the shortening of mental processes brought about by practice is largely due to the power of doing two things at once, is an

overlapping of mental processes; we know, too, that when processes become automatic they may accompany more deliberate and reasoned processes without interference; and we further recognize that certain processes directed to a common end are almost as easily performed together as separately. On the other hand we observe that states of extreme concentration are characterized by immobility, even by a slackening of the automatic functions; we observe the various kinds of disturbance all indicating the interference of two or more mental processes; and we appreciate the necessity of dividing our work into small parts so that they may be easily absorbed and not over-tax our powers. In entering upon this general problem, we at once encounter the difficulty of defining the mental unit; what is a mental process? In a certain sense we are always doing two things at once; the rhythmical functions of circulation and respiration go on while we work; we walk and think, we eat and talk, we write and listen at the same time. In every game of skill several senses act at once; the eye and hand, the ear and mouth, taste and smell act together and aid one another. On the other hand, however, in an intense attention to some fascinating event we stand motionless and almost stop breathing; many persons when thoroughly interested while talking upon the street involuntarily slacken their pace, or stop altogether; few of those who illustrate their remarks by off-hand sketches can talk and draw at the same time, and so on. Our general inquiry is "What processes hinder, what aid one another;" the present study makes no attempt to answer this most important query, but simply describes a few facts and suggestions relating to a very small and special portion of the general field.

We choose as the two types of process, (1) the performance of finger movements, involving rhythm and counting, and (2) of such processes as adding and reading under various conditions. The former were written (by the usual method of a system of Marcy tambours) upon a rotating cylinder, while for the latter we simply noted the time of a set task, performed as rapidly as possible. Our records are in no case very full, and the conclusions drawn are suggestive rather than final. We will consider first the effect upon the movement of an accompanying mental task.

The chief movements used were:

(1) A regular beating with the finger at any rate the subject chose; this we speak of as an *ad libitum* movement.

(2) A movement as rapid as possible and still regular; this is a maximum movement.

(3) Beating in groups of 2s, 3s, 4s, 5s or more.

(4) Beating in alternate groups of 3s and 2s, and 6s, 4s and 2s.

(5) Keeping time to a metronome at different rates, to an air hummed to oneself, etc.

The method by which the effect of mental tasks upon these movements was estimated was to compare the *ease*, the *regularity* and the *time* of these movements when accompanied and when unaccompanied by mental operations. Our results are not sufficiently numerous to show carefully all those effects (time, ease and regularity), but in general certain tendencies are evident. The ease is shown not alone by the feeling of difficulty, but as well by the presence of errors, varying in kind and degree; so, too, even when the rhythm is maintained, it may be more or less irregular, and in turn this irregularity manifests itself in a slowing of the movements. This slowing up is the natural accompaniment of difficult processes. It will thus be seen that these three indications are closely connected with one another, each being in a measure indicative of the others and all evidencing the same points. The "normals" or times of movements with no accompanying mental process are naturally variable. The records upon six days for J. J. of an *ad libitum* movement were 335 σ, 320 σ, 318 σ, 518 σ, 388 σ, 424 σ, 326 σ, while, when several records were taken in the same day, the variations were much slighter in extent. The rate of maximum movements is much more constant, as the following records (of J. S.) show: 152, 163, 140, 148, 160, 164 σ. For beating in groups of 5 the records (of J. J.) have the following times: 1837, 1966, 1801, 1734, 1471 σ, and so on. These figures may perhaps suffice to illustrate the range of constancy of the phenomena in question.

Our first query will be: How far (neglecting for the moment the nature of the accompanying mental operation) will various movements be interfered with by the accompanying process? Our facts suggest the conclusion that the simpler movements are less interfered with than the more complex ones; the records of *ad libitum* movements show no appreciable difference when accompanied or when unaccompanied by other tasks; maximum movements are always somewhat slackened by the accompanying task; beating in groups of 2s, 3s, 4s or 5s become successively more and more interfered with by accompanying mental processes, such interference appearing not very much in a modification of time, but in the irregularity, the presence of errors (there being as a rule more beats in a group then there should be) and in the feeling of strain; in such movements as beating in groups of eleven, of alternate 3s and 2s or 6s, 4s and 2s, frequent failures set in, and when the result is fairly successful, the time is increased and the record more or less irregular. We are unable to range the

various movements in their order of relative difficulty by the amount of interference, but the extremes are very markedly differentiated.

Our second query relates to the amount of interference of different mental tasks. Reading words in construction, reading words disconnected, reading numbers and adding numbers were the chief types of processes used; of these, reading words in sentences is by far the easiest task, all the others tending to make the subject have each beat coincide with a word or addition, and thus slowing the process. Furthermore, any of the movements involving counting, (particularly alternating 3s and 2s and the like) were more interfered with by adding than by reading. But the most striking difference depends upon the manner of going through the mental process, that is, whether the reading, etc., is done aloud or to oneself. In the former case the interference sets in much sooner and is much more serious than in the latter. Even quite simple movements are rendered irregular by reading or adding aloud; and such movements as beating in 3s and 2s or 6s, 4s and 2s were practically failures in such a case, though very successfully done with silent reading. An intermediate process of mumbling seemed to yield an intermediate degree of difficulty. The interference manifests itself clearly in an increased effort, a great irregularity and presence of errors, and a lengthening of the time of movement. Motor processes thus seem to interfere with motor ones, while refraining from movement during intellectual effort would be helpful. Passing now to the effect of an accompanying movement upon the time of such operations as reading sentences, words or numbers, adding (both aloud and to oneself); our data are meagre, but the following suggested inferences, together with the facts that suggest them, may be noted.

(1) The time needed to perform these mental processes is distinctly increased by such accompanying movements, the extent of the increase depending upon the complexity of the movement. (The general average of all the records (107) shows an increase of 4.28 seconds or 30.8 per cent.; J. J., 6.5 seconds or 26.5 per cent.; W. B. C., 6.02 seconds or 36.6 per cent.)

(2) Comparing the process of adding with that of reading, the former is the more complex, and seems to be more interfered with by the accompanying movements. (Comparable records are only about a half-dozen of J. J.'s in which the percentages of increase are about as 40 per cent. to 30 per cent.

(3) Reading and adding aloud are slightly more interfered with by the movements than the same processes performed to oneself. (In six dozen records of J. J., the percentages of in-

crease in the two cases are 31 per cent. and 24 per cent.; in W. B. C., the result is obscured by other factors.)

(4) Of the effect of different kinds of accompanying movements the following may be mentioned.

(a) If the movements are rhythmical beats arranged in groups, like a line of verse or a measure of music, the time increases with the number of beats in a group. For W. B. C., with groups of 2, 3, 4, 5, 6, the times of reading the same passages were 10.4, 11.0, 13.8, 14.0, 15.4 seconds. In one case groups of eleven were attempted with an increase above the normal of about 80 per cent. A similar result appears, too, in attempting to keep time to a beating metronome every 2d, 3d, 4th or 6th stroke of which is marked by a bell, with the accented syllable to coincide with the stroke of the bell.

(b) Simple regular beating, whether to the accompaniment of a metronome or without, can be done without increase of time for reading or adding; for J. J. this is true independently of the rate of the interval. Indeed there is some evidence that a maximum rate of beating also hurries up the mental process. The movements that retarded the processes most were beating in groups of eleven, making three beats of the right hand correspond to one of the left, and beating in groups formed by a six, a four and a two in turn.

(5) Reading disconnected words is more interfered with than reading words forming sense; part of which is due to the tendency of making each word correspond to the beat. While all these points require further corroboration, our results are sufficiently suggestive to evidence the promise of research in this direction. The next step would be to make a detailed study of a few types of interference and accumulate sufficient records to allow of quantitative expression. This it is hoped will be undertaken upon some future occasion.